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INTRODUCTORY LECTURE  
ON  
EXPERIMENTAL PHYSICS

OCTOBER 25, 1871.

BY

J. CLERK MAXWELL, M.A.

LATE FELLOW OF TRINITY COLLEGE; LL.D. EDIN.; F.R.SS. L. & E.;  
PROFESSOR OF EXPERIMENTAL PHYSICS IN THE UNIVERSITY OF CAMBRIDGE.

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# INTRODUCTORY LECTURE

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## EXPERIMENTAL PHYSICS.

THE University of Cambridge, in accordance with that law of its evolution, by which, while maintaining the strictest continuity between the successive phases of its history, it adapts itself with more or less promptness to the requirements of the times, has lately instituted a course of Experimental Physics. This course of study, while it requires us to maintain in action all those powers of attention and analysis which have been so long cultivated in the University, calls on us to exercise our senses in observation, and our hands in manipulation. The familiar apparatus of pen, ink, and paper will no longer be sufficient for us, and we shall require more room than that afforded by a seat at a desk, and a wider area than that of the black board. We owe it to the munificence of our Chancellor, that, whatever be the character in other respects of the experiments which we hope hereafter to conduct, the material facilities for their full development will be upon a scale which has not hitherto been surpassed.

The main feature, therefore, of Experimental Physics at Cambridge is the Devonshire Physical Laboratory, and I think it desirable that on the present occasion, before we enter on the details of any special study, we should consider by what means we, the University of Cambridge, may, as a living body, appropriate and vitalize this new organ, the outward shell of which we expect soon to rise



before us. The course of study at this University has always included Natural Philosophy, as well as Pure Mathematics. To diffuse a sound knowledge of Physics, and to imbue the minds of our students with correct dynamical principles, have been long regarded as among our highest functions, and very few of us can now place ourselves in the mental condition in which even such philosophers as the great Descartes were involved in the days before Newton had announced the true laws of the motion of bodies. Indeed the cultivation and diffusion of sound dynamical ideas has already effected a great change in the language and thoughts even of those who make no pretensions to science, and we are daily receiving fresh proofs that the popularization of scientific doctrines is producing as great an alteration in the mental state of society as the material applications of science are effecting in its outward life. Such indeed is the respect paid to science, that the most absurd opinions may become current, provided they are expressed in language, the sound of which recalls some well-known scientific phrase. If society is thus prepared to receive all kinds of scientific doctrines, it is our part to provide for the diffusion and cultivation, not only of true scientific principles, but of a spirit of sound criticism, founded on an examination of the evidences on which statements apparently scientific depend.

When we shall be able to employ in scientific education, not only the trained attention of the student, and his familiarity with symbols, but the keenness of his eye, the quickness of his ear, the delicacy of his touch, and the adroitness of his fingers, we shall not only extend our influence over a class of men who are not fond of cold abstractions, but, by opening at once all the gateways of knowledge, we shall ensure the association of the doctrines of science with those elementary sensations which form the obscure background of all our conscious thoughts, and which lend a vividness and relief to ideas, which, when presented as mere abstract terms, are apt to fade entirely from the memory.

In a course of Experimental Physics we may consider either the Physics or the Experiments as the leading feature. We may either employ the experiments to illustrate the phenomena of a particular branch of Physics, or we may make some physical research in order to exemplify a particular experimental method. In the order of time, we should begin, in the Lecture Room, with a course of lectures on some branch of Physics aided by experiments of illustration, and conclude, in the Laboratory, with a course of experiments of research.

Let me say a few words on these two classes of experiments,—Experiments of Illustration and Experiments of Research. The aim of an experiment of illustration is to throw light upon some scientific idea so that the student may be enabled to grasp it. The circumstances of the experiment are so arranged that the phenomenon which we wish to observe or to exhibit is brought into prominence, instead of being obscured and entangled among other phenomena, as it is when it occurs in the ordinary course of nature. To exhibit illustrative experiments, to encourage others to make them, and to cultivate in every way the ideas on which they throw light, forms an important part of our duty. The simpler the materials of an illustrative experiment, and the more familiar they are to the student, the more thoroughly is he likely to acquire the idea which it is meant to illustrate. The educational value of such experiments is often inversely proportional to the complexity of the apparatus. The student who uses home-made apparatus, which is always going wrong, often learns more than one who has the use of carefully adjusted instruments, to which he is apt to trust, and which he dares not take to pieces.

It is very necessary that those who are trying to learn from books the facts of physical science should be enabled by the help of a few illustrative experiments to recognise these facts when they meet with them out of doors. Science appears to us with a very different aspect after we have found out that it is not in lecture rooms only, and by

means of the electric light projected on a screen, that we may witness physical phenomena, but that we may find illustrations of the highest doctrines of science in games and gymnastics, in travelling by land and by water, in storms of the air and of the sea, and wherever there is matter in motion.

This habit of recognising principles amid the endless variety of their action can never degrade our sense of the sublimity of nature, or mar our enjoyment of its beauty. On the contrary, it tends to rescue our scientific ideas from that vague condition in which we too often leave them, buried among the other products of a lazy credulity, and to raise them into their proper position among the doctrines in which our faith is so assured, that we are ready at all times to act on them.

Experiments of illustration may be of very different kinds. Some may be adaptations of the commonest operations of ordinary life, others may be carefully arranged exhibitions of some phenomenon which occurs only under peculiar conditions. They all, however, agree in this, that their aim is to present some phenomenon to the senses of the student in such a way that he may associate with it the appropriate scientific idea. When he has grasped this idea, the experiment which illustrates it has served its purpose.

In an experiment of research, on the other hand, this is not the principal aim. It is true that an experiment, in which the principal aim is to see what happens under certain conditions, may be regarded as an experiment of research by those who are not yet familiar with the result, but in experimental researches, strictly so called, the ultimate object is to measure something which we have already seen—to obtain a numerical estimate of some magnitude.

Experiments of this class—those in which measurement of some kind is involved, are the proper work of a Physical Laboratory. In every experiment we have first to make our senses familiar with the phenomenon, but we must

not stop here, we must find out which of its features are capable of measurement, and what measurements are required in order to make a complete specification of the phenomenon. We must then make these measurements, and deduce from them the result which we require to find.

This characteristic of modern experiments—that they consist principally of measurements,—is so prominent, that the opinion seems to have got abroad, that in a few years all the great physical constants will have been approximately estimated, and that the only occupation which will then be left to men of science will be to carry on these measurements to another place of decimals.

If this is really the state of things to which we are approaching, our Laboratory may perhaps become celebrated as a place of conscientious labour and consummate skill, but it will be out of place in the University, and ought rather to be classed with the other great workshops of our country, where equal ability is directed to more useful ends.

But we have no right to think thus of the unsearchable riches of creation, or of the untried fertility of those fresh minds into which these riches will continue to be poured. It may possibly be true that, in some of those fields of discovery which lie open to such rough observations as can be made without artificial methods, the great explorers of former times have appropriated most of what is valuable, and that the gleanings which remain are sought after, rather for their abstruseness, than for their intrinsic worth. But the history of science shews that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers. I might bring forward instances gathered from every branch of science, showing how the labour of careful measurement has been rewarded by the discovery of new fields of

research, and by the development of new scientific ideas. But the history of the science of terrestrial magnetism affords us a sufficient example of what may be done by Experiments in Concert, such as we hope some day to perform in our Laboratory.

That celebrated traveller, Humboldt, was profoundly impressed with the scientific value of a combined effort to be made by the observers of all nations, to obtain accurate measurements of the magnetism of the earth; and we owe it mainly to his enthusiasm for science, his great reputation and his wide-spread influence, that not only private men of science, but the governments of most of the civilized nations, our own among the number, were induced to take part in the enterprize. But the actual working out of the scheme, and the arrangements by which the labours of the observers were so directed as to obtain the best results, we owe to the great mathematician Gauss, working along with Weber, the future founder of the science of electro-magnetic measurement, in the magnetic observatory of Göttingen, and aided by the skill of the instrument-maker Leyser. These men, however, did not work alone. Numbers of scientific men joined the Magnetic Union, learned the use of the new instruments and the new methods of reducing the observations; and in every city of Europe you might see them, at certain stated times, sitting, each in his cold wooden shed, with his eye fixed at the telescope, his ear attentive to the clock, and his pencil recording in his note-book the instantaneous position of the suspended magnet.

Bacon's conception of "Experiments in concert" was thus realized, the scattered forces of science were converted into a regular army, and emulation and jealousy became out of place, for the results obtained by any one observer were of no value till they were combined with those of the others.

The increase in the accuracy and completeness of magnetic observations which was obtained by the new method, opened up fields of research which were hardly

suspected to exist by those whose observations of the magnetic needle had been conducted in a more primitive manner. We must reserve for its proper place in our course any detailed description of the disturbances to which the magnetism of our planet is found to be subject. Some of these disturbances are periodic, following the regular courses of the sun and moon. Others are sudden, and are called magnetic storms, but, like the storms of the atmosphere, they have their known seasons of frequency. The last and the most mysterious of these magnetic changes is that secular variation by which the whole character of the earth, as a great magnet, is being slowly modified, while the magnetic poles creep on, from century to century, along their winding track in the polar regions.

We have thus learned that the interior of the earth is subject to the influences of the heavenly bodies, but that besides this there is a constantly progressive change going on, the cause of which is entirely unknown. In each of the magnetic observatories throughout the world an arrangement is at work, by means of which a suspended magnet directs a ray of light on a prepared sheet of paper moved by clockwork. On that paper the never-resting heart of the earth is now tracing, in telegraphic symbols which will one day be interpreted, a record of its pulsations and its flutterings, as well as of that slow but mighty working which warns us that we must not suppose that the inner history of our planet is ended.

But this great experimental research on Terrestrial Magnetism produced lasting effects on the progress of science in general. I need only mention one or two instances. The new methods of measuring forces were successfully applied by Weber to the numerical determination of all the phenomena of electricity, and very soon afterwards the electric telegraph, by conferring a commercial value on exact numerical measurements, contributed largely to the advancement, as well as to the diffusion of scientific knowledge.

But it is not in these more modern branches of science

alone that this influence is felt. It is to Gauss, to the Magnetic Union, and to magnetic observers in general, that we owe our deliverance from that absurd method of estimating forces by a variable standard which prevailed so long even among men of science. It was Gauss who first based the practical measurement of magnetic force (and therefore of every other force) on those long established principles, which, though they are embodied in every dynamical equation, have been so generally set aside, that these very equations, though correctly given in our Cambridge textbooks, are usually explained there by assuming, in addition to the variable standard of force, a variable, and therefore illegal, standard of mass.

Such, then, were some of the scientific results which followed in this case from bringing together mathematical power, experimental sagacity, and manipulative skill, to direct and assist the labours of a body of zealous observers. If therefore we desire, for our own advantage and for the honour of our University, that the Devonshire Laboratory should be successful, we must endeavour to maintain it in living union with the other organs and faculties of our learned body. We shall therefore first consider the relation in which we stand to those mathematical studies which have so long flourished among us, which deal with our own subjects, and which differ from our experimental studies only in the mode in which they are presented to the mind.

There is no more powerful method for introducing knowledge into the mind than that of presenting it in as many different ways as we can. When the ideas, after entering through different gateways, effect a junction in the citadel of the mind, the position they occupy becomes impregnable. Opticians tell us that the mental combination of the views of an object which we obtain from stations no further apart than our two eyes is sufficient to produce in our minds an impression of the solidity of the object seen; and we find that this impression is produced even when we are aware that we are really looking at two flat

pictures placed in a stereoscope. It is therefore natural to expect that the knowledge of physical science obtained by the combined use of mathematical analysis and experimental research will be of a more solid, available, and enduring kind than that possessed by the mere mathematician or the mere experimenter.

But what will be the effect on the University, if men pursuing that course of reading which has produced so many distinguished Wranglers, turn aside to work experiments? Will not their attendance at the Laboratory count not merely as time withdrawn from their more legitimate studies, but as the introduction of a disturbing element, tainting their mathematical conceptions with material imagery, and sapping their faith in the formulæ of the textbooks? Besides this, we have already heard complaints of the undue extension of our studies, and of the strain put upon our questionists by the weight of learning which they try to carry with them into the Senate-House. If we now ask them to get up their subjects not only by books and writing, but at the same time by observation and manipulation, will they not break down altogether? The Physical Laboratory, we are told, may perhaps be useful to those who are going out in Natural Science, and who do not take in Mathematics, but to attempt to combine both kinds of study during the time of residence at the University is more than one mind can bear.

No doubt there is some reason for this feeling. Many of us have already overcome the initial difficulties of mathematical training. When we now go on with our study, we feel that it requires exertion and involves fatigue, but we are confident that if we only work hard our progress will be certain.

Some of us, on the other hand, may have had some experience of the routine of experimental work. As soon as we can read scales, observe times, focus telescopes, and so on, this kind of work ceases to require any great mental effort. We may perhaps tire our eyes and weary our backs, but we do not greatly fatigue our minds.



It is not till we attempt to bring the theoretical part of our training into contact with the practical that we begin to experience the full effect of what Faraday has called "mental inertia"—not only the difficulty of recognising, among the concrete objects before us, the abstract relation which we have learned from books, but the distracting pain of wrenching the mind away from the symbols to the objects, and from the objects back to the symbols. This however is the price we have to pay for new ideas.

But when we have overcome these difficulties, and successfully bridged over the gulph between the abstract and the concrete, it is not a mere piece of knowledge that we have obtained : we have acquired the rudiment of a permanent mental endowment. When, by a repetition of efforts of this kind, we have more fully developed the scientific faculty, the exercise of this faculty in detecting scientific principles in nature, and in directing practice by theory, is no longer irksome, but becomes an unfailing source of enjoyment, to which we return so often, that at last even our careless thoughts begin to run in a scientific channel.

I quite admit that our mental energy is limited in quantity, and I know that many zealous students try to do more than is good for them. But the question about the introduction of experimental study is not entirely one of quantity, It is to a great extent a question of distribution of energy. Some distributions of energy, we know, are more useful than others, because they are more available for those purposes which we desire to accomplish.

Now in the case of study, a great part of our fatigue often arises, not from those mental efforts by which we obtain the mastery of the subject, but from those which are spent in recalling our wandering thoughts ; and these efforts of attention would be much less fatiguing if the disturbing force of mental distraction could be removed.

This is the reason why a man whose soul is in his work always makes more progress than one whose aim is something not immediately connected with his occupation. In the latter case the very motive of which he makes use to

stimulate his flagging powers becomes the means of distracting his mind from the work before him.

There may be some mathematicians who pursue their studies entirely for their own sake. Most men, however, think that the chief use of mathematics is found in the interpretation of nature. Now a man who studies a piece of mathematics in order to understand some natural phenomenon which he has seen, or to calculate the best arrangement of some experiment which he means to make, is likely to meet with far less distraction of mind than if his sole aim had been to sharpen his mind for the successful practice of the Law, or to obtain a high place in the Mathematical Tripos.

I have known men, who when they were at school, never could see the good of mathematics, but who, when in after life they made this discovery, not only became eminent as scientific engineers, but made considerable progress in the study of abstract mathematics. If our experimental course should help any of you to see the good of mathematics, it will relieve us of much anxiety, for it will not only ensure the success of your future studies, but it will make it much less likely that they will prove injurious to your health.

But why should we labour to prove the advantage of practical science to the University? Let us rather speak of the help which the University may give to science, when men well trained in mathematics and enjoying the advantages of a well-appointed Laboratory, shall unite their efforts to carry out some experimental research which no solitary worker could attempt.

At first it is probable that our principal experimental work must be the illustration of particular branches of science, but as we go on we must add to this the study of scientific methods, the same method being sometimes illustrated by its application to researches belonging to different branches of science.

We might even imagine a course of experimental study the arrangement of which should be founded on a classifi-

cation of methods, and not on that of the objects of investigation. A combination of the two plans seems to me better than either, and while we take every opportunity of studying methods, we shall take care not to dissociate the method from the scientific research to which it is applied, and to which it owes its value.

We shall therefore arrange our lectures according to the classification of the principal natural phenomena, such as heat, electricity, magnetism and so on.

In the laboratory, on the other hand, the place of the different instruments will be determined by a classification according to methods, such as weighing and measuring, observations of time, optical and electrical methods of observation, and so on.

The determination of the experiments to be performed at a particular time must often depend upon the means we have at command, and in the case of the more elaborate experiments, this may imply a long time of preparation, during which the instruments, the methods, and the observers themselves, are being gradually fitted for their work. When we have thus brought together the requisites, both material and intellectual, for a particular experiment, it may sometimes be desirable that before the instruments are dismantled and the observers dispersed, we should make some other experiment, requiring the same method, but dealing perhaps with an entirely different class of physical phenomena.

Our principal work, however, in the Laboratory must be to acquaint ourselves with all kinds of scientific methods, to compare them, and to estimate their value. It will, I think, be a result worthy of our University, and more likely to be accomplished here than in any private laboratory, if, by the free and full discussion of the relative value of different scientific procedures, we succeed in forming a school of scientific criticism, and in assisting the development of the doctrine of method.

But admitting that a practical acquaintance with the methods of Physical Science is an essential part of a ma-

thematical and scientific education, we may be asked whether we are not attributing too much importance to science altogether as part of a liberal education.

Fortunately, there is no question here whether the University should continue to be a place of liberal education, or should devote itself to preparing young men for particular professions. Hence though some of us may, I hope, see reason to make the pursuit of science the main business of our lives, it must be one of our most constant aims to maintain a living connexion between our work and the other liberal studies of Cambridge, whether literary, philological, historical or philosophical.

There is a narrow professional spirit which may grow up among men of science, just as it does among men who practise any other special business. But surely a University is the very place where we should be able to overcome this tendency of men to become, as it were, granulated into small worlds, which are all the more worldly for their very smallness. We lose the advantage of having men of varied pursuits collected into one body, if we do not endeavour to imbibe some of the spirit even of those whose special branch of learning is different from our own.

It is not so long ago since any man who devoted himself to geometry, or to any science requiring continued application, was looked upon as necessarily a misanthrope, who must have abandoned all human interests, and betaken himself to abstractions so far removed from the world of life and action that he has become insensible alike to the attractions of pleasure and to the claims of duty.

In the present day, men of science are not looked upon with the same awe or with the same suspicion. They are supposed to be in league with the material spirit of the age, and to form a kind of advanced Radical party among men of learning.

We are not here to defend literary and historical studies. We admit that the proper study of mankind is man. But is the student of science to be withdrawn from the study of man, or cut off from every noble feeling, so long

as he lives in intellectual fellowship with men who have devoted their lives to the discovery of truth, and the results of whose enquiries have impressed themselves on the ordinary speech and way of thinking of men who never heard their names? Or is the student of history and of man to omit from his consideration the history of the origin and diffusion of those ideas which have produced so great a difference between one age of the world and another?

It is true that the history of science is very different from the science of history. We are not studying or attempting to study the working of those blind forces which, we are told, are operating on crowds of obscure people, shaking principalities and powers, and compelling reasonable men to bring events to pass in an order laid down by philosophers.

The men whose names are found in the history of science are not mere hypothetical constituents of a crowd, to be reasoned upon only in masses. We recognise them as men like ourselves, and their actions and thoughts, being more free from the influence of passion, and recorded more accurately than those of other men, are all the better materials for the study of the calmer parts of human nature.

But the history of science is not restricted to the enumeration of successful investigations. It has to tell of unsuccessful enquiries, and to explain why some of the ablest men have failed to find the key of knowledge, and how the reputation of others has only given a firmer footing to the errors into which they fell.

The history of the development, whether normal or abnormal, of ideas is of all subjects that in which we, as thinking men, take the deepest interest. But when the action of the mind passes out of the intellectual stage, in which truth and error are the alternatives, into the more violently emotional states of anger and passion, malice and envy, fury and madness; the student of science, though he is obliged to recognise the powerful influence which these wild forces have exercised on mankind, is perhaps in some

measure disqualified from pursuing the study of this part of human nature.

But then how few of us are capable of deriving profit from such studies. We cannot enter into full sympathy with these lower phases of our nature without losing some of that antipathy to them which is our surest safeguard against a reversion to a meaner type, and we gladly return to the company of those illustrious men who by aspiring to noble ends, whether intellectual or practical, have risen above the region of storms into a clearer atmosphere, where there is no misrepresentation of opinion, nor ambiguity of expression, but where one mind comes into closest contact with another at the point where both approach nearest to the truth.

I propose to lecture during this term on Heat, and, as our facilities for experimental work are not yet fully developed, I shall endeavour to place before you the relative position and scientific connexion of the different branches of the science, rather than to discuss the details of experimental methods.

We shall begin with Thermometry, or the registration of temperatures, and Calorimetry, or the measurement of quantities of heat. We shall then go on to Thermodynamics, which investigates the relations between the thermal properties of bodies and their other dynamical properties, in so far as these relations may be traced without any assumption as to the particular constitution of these bodies.

The principles of Thermodynamics throw great light on all the phenomena of nature, and it is probable that many valuable applications of these principles have yet to be made; but we shall have to point out the limits of this science, and to show that many problems in nature, especially those in which the Dissipation of Energy comes into play, are not capable of solution by the principles of Thermodynamics alone, but that in order to understand them, we are obliged to form some more definite theory of the constitution of bodies.

Two theories of the constitution of bodies have struggled for victory with various fortunes since the earliest ages of speculation : one is the theory of a universal plenum, the other is that of atoms and void.

The theory of the plenum is associated with the doctrine of mathematical continuity, and its mathematical methods are those of the Differential Calculus, which is the appropriate expression of the relations of continuous quantity.

The theory of atoms and void leads us to attach more importance to the doctrines of integral numbers and definite proportions; but, in applying dynamical principles to the motion of immense numbers of atoms, the limitation of our faculties forces us to abandon the attempt to express the exact history of each atom, and to be content with estimating the average condition of a group of atoms large enough to be visible. This method of dealing with groups of atoms, which I may call the statistical method, and which in the present state of our knowledge is the only available method of studying the properties of real bodies, involves an abandonment of strict dynamical principles, and an adoption of the mathematical methods belonging to the theory of probability. It is probable that important results will be obtained by the application of this method, which is as yet little known and is not familiar to our minds. If the actual history of Science had been different, and if the scientific doctrines most familiar to us had been those which must be expressed in this way, it is possible that we might have considered the existence of a certain kind of contingency a self-evident truth, and treated the doctrine of philosophical necessity as a mere sophism.

About the beginning of this century, the properties of bodies were investigated by several distinguished French mathematicians on the hypothesis that they are systems of molecules in equilibrium. The somewhat unsatisfactory nature of the results of these investigations produced, especially in this country, a reaction in favour of

the opposite method of treating bodies as if they were, so far at least as our experiments are concerned, truly continuous. This method, in the hands of Green, Stokes, and others, has led to results, the value of which does not at all depend on what theory we adopt as to the ultimate constitution of bodies.

One very important result of the investigation of the properties of bodies on the hypothesis that they are truly continuous is that it furnishes us with a test by which we can ascertain, by experiments on a real body, to what degree of tenuity it must be reduced before it begins to give evidence that its properties are no longer the same as those of the body in mass. Investigations of this kind, combined with a study of various phenomena of diffusion and of dissipation of energy, have recently added greatly to the evidence in favour of the hypothesis that bodies are systems of molecules in motion.

I hope to be able to lay before you in the course of the term some of the evidence for the existence of molecules, considered as individual bodies having definite properties. The molecule, as it is presented to the scientific imagination, is a very different body from any of those with which experience has hitherto made us acquainted.

In the first place its mass, and the other constants which define its properties, are absolutely invariable; the individual molecule can neither grow nor decay, but remains unchanged amid all the changes of the bodies of which it may form a constituent.

In the second place it is not the only molecule of its kind, for there are innumerable other molecules, whose constants are not approximately, but absolutely identical with those of the first molecule, and this whether they are found on the earth, in the sun, or in the fixed stars.

By what process of evolution the philosophers of the future will attempt to account for this identity in the properties of such a multitude of bodies, each of them unchangeable in magnitude, and some of them separated from others by distances which Astronomy attempts in vain to



measure, I cannot conjecture. My mind is limited in its power of speculation, and I am forced to believe that these molecules must have been made as they are from the beginning of their existence.

I also conclude that since none of the processes of nature, during their varied action on different individual molecules, have produced, in the course of ages, the slightest difference between the properties of one molecule and those of another, the history of whose combinations has been different, we cannot ascribe either their existence or the identity of their properties to the operation of any of those causes which we call natural.

Is it true then that our scientific speculations have really penetrated beneath the visible appearance of things, which seem to be subject to generation and corruption, and reached the entrance of that world of order and perfection, which continues this day as it was created, perfect in number and measure and weight?

We may be mistaken. No one has as yet seen or handled an individual molecule, and our molecular hypothesis may, in its turn, be supplanted by some new theory of the constitution of matter; but the idea of the existence of unnumbered individual things, all alike and all unchangeable, is one which cannot enter the human mind and remain without fruit.

But what if these molecules, indestructible as they are, turn out to be not substances themselves, but mere affections of some other substance?

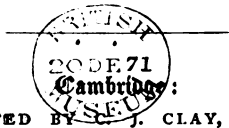
According to Sir W. Thomson's theory of Vortex Atoms, the substance of which the molecule consists is a uniformly dense *plenum*, the properties of which are those of a perfect fluid, the molecule itself being nothing but a certain motion impressed on a portion of this fluid, and this motion is shewn, by a theorem due to Helmholtz, to be as indestructible as we believe a portion of matter to be.

If a theory of this kind is true, or even if it is conceivable, our idea of matter may have been introduced into our minds through our experience of those systems of vortices

which we call bodies, but which are not substances, but motions of a substance ; and yet the idea which we have thus acquired of matter, as a substance possessing inertia, may be truly applicable to that fluid of which the vortices are the motion, but of whose existence, apart from the vortical motion of some of its parts, our experience gives us no evidence whatever.

It has been asserted that metaphysical speculation is a thing of the past, and that physical science has extirpated it. The discussion of the categories of existence, however, does not appear to be in danger of coming to an end in our time, and the exercise of speculation continues as fascinating to every fresh mind as it was in the days of Thales.

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